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Interactive comment on "Response of winter fine particulate matter concentrations to emission and meteorology changes in North China" *by* M. Gao et al.

Anonymous Referee #2

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Overall I'd rate this manuscript as minor revisions - some rewriting is required, but I have no major concerns, aside from the missing SOA in the regional model. This is a significant omission - not so big that I'd require the authors to repeat their simulations with this fixed, but they definitely need to explain its absence and how this is justified.

My largest concern: Page 10, lines 14-15. I find the omission of a secondary organic aerosol formation mechanism in the model a concern, given the large amount of work in the literature showing the importance of this process towards net aerosol production. The authors reference Gao et al 2016 mentioning its impact is small – this reasoning should be repeated here. It's potentially a significant omission, given the increases in the region's VOC emissions – what are the unique local conditions that allow the au-

thors to justify its omission? Given the availability of several different parameterizations for SOA formation in the literature, why did the authors not ("just") include one in their model?

Not a "concern" per se, but something worth pointing out to the readers and perhaps investigating a bit further. Three points, all related:

(1) Page 7, lines 14 through 17: Later in the discussion the authors mention the manner in which this was done, by perturbing the initial and boundary conditions of the individual meteorological components to create these changes. This should be mentioned here as well, in a single sentence. This methodology later seems to result in a response from the fully coupled model which counteracts the meteorological perturbations. Some discussion of the mechanisms by which these counteracting effects takes place would be warranted.

(2) Sections 3.3,3.4, 3.5. The impact of these changes in the meteorology initial and boundary conditions may themselves be partially due to the response of the aerosols through feedbacks; affecting radiative transfer, etc. When the IC and BC temperature increases, the domain temperature decreases (section 3.3). When the IC and BC RH increases, the domain RH decreases (section 3.4). When the IC and BC wind speeds increase, the domain wind speeds decrease (question: section 3.5: I assume that the first sentence should read "carried out when initial and boundary condition wind speeds were increased"? Please explain in more detail how the winds were perturbed, and whether this was surface or 3D winds.

(3) Each of these meteorological perturbations to the initial and boundary conditions resulted in a model response which acted counter to that change. Would the authors concur that the feedback meteorological system is acting to damp or counteract meteorological perturbations? This is something worth mentioning in the paper, along with how the feedbacks act this way (e.g. temperature increases leading to increases in the type of aerosols which reduce surface temperatures, etc.).

Relatively minor issues:

Page 2, lines 19-20. The statement regarding long-term trends in atmospheric circulation potentially being important due to PM2.5's sensitivity to wind speed and aerosol feedbacks is unclear; it's not clear how the latter imply the former. I'm not sure, having scanned through the paper, that the authors have made a good case for that connection.

There are frequent references to Gao et al 2016 (perhaps intended as a companion paper) – for the benefit of those who do not have this paper, things like "domain 2 in Gao et al, 2016) (page 7 line 19) should be given more explicitly in this work (e.g. by showing the region on one of the figures). Similarly (Page 9, line 22), readers unfamiliar with the geography of the region might benefit from some symbols with the locations of the cities and regions mentioned appearing on the maps (maps are too small for names appearing on the maps themselves)

Page 3, line 22: would be better as "increases sulfate concentration due to the temperature dependence of SO2 oxidation and resulting higher SO2 oxidation rates".

Page 6 line 16: Please describe how the VOC emissions are speciated into CBM-Z VOCs in this description. That is, a speciation profiles must have been used – are these specific for different emissions sources, more generic, etc.? Or are the emissions data used already pre-speciated into the individual VOCs required for the model's chemical mechanism?

Page 8 line 19. There is recent work by McLinden et al in Nature Geoscience (May 2016), which uses satellite-based estimates of SO2 emissions to show regions and particular large sources which have been underreported in emissions inventories. Do the regions this reference shows have underestimates in SO2 production spatially correspond to the regions the authors of the current work have shown have underestimates in sulphate? If so, this would be worth mentioning.

Figure 3 discussion on page 10: to what extent do boundary conditions account for these changes? How much do the boundary conditions change between the two simulations? This should be discussed in the manuscript.

Page 12, line 18 to Page 13, line 11: Please include a plot of VOC:NOx ratios at the start and the end of the period to show how the ratio has changed in response to the emissions changes.

Page 15, lines 8-14: The OH increase has been attributed to the temperature perturbation (which makes sense in that this is the boundary condition which has been changed), but this does not necessarily mean that the temperature-dependent reaction rates are the main pathway by which temperature has increased the OH concentration. Another possible path might be through decreases in cloudiness leading to increases in photolysis, leading to increases in OH. Were there any changes in cloudiness in response to the temperature perturbation (or is this meteorological perturbation not fully interactive in which case, yes, temperature alone could be responsible for the OH change)?

Page 19, lines 21 to 23 versus Page 20, lines 10 through 12. In the first set of lines, the authors recommend reductions in SO2 and NH3 as a means of reduction particulate matter; in the second set of lines they show how increases in NH3 and NOx result in particle nitrate formation increasing in the future, despite SO2 decreases, in the winter. The authors need to clarify why / how NH3 is more important for future reductions of PM2.5 than NOx. Is the region relatively ammonia-poor, hence particle nitrate formation will be controlled by NH3 rather than NOx levels? The first set of statements needs to be justified, given the second set of statements, which could be due to either or both of the changes in NH3 and NOx.

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